a monograph with figures, by M. Bourguingnat, of all the mollusca of this age found in the Seine Basin. This well-known conchologist makes out that out of a total of 76 there are 38 new species which he considers as extinct, a conclusion which we expect English conchologists will hardly be prepared to agree with, as they have detected no extinct species in these deposits, and find only a few which are not local—a view in which we also believe most French conchologists join. The author considers that the same mammalian fauna is common to both the high-level and the low-level gravels. In one main point, however, do these gravels differ. In those of the high-levels of Montreuil and Bicêtre no Human remains, no Flint Implements, have been found, whereas, in those of the low-levels of Clichy, Grenelle, &c., above 5,000 flints, more or less worked, are stated to have been found by a single collector. Besides these works of early Man, M. Belgrand states that human bones, skulls, and entire skeletons, have been found in these lower gravels; but it seems to us that much of this evidence requires confirmation.

The Quaternary period of the Seine Basin is coeval, in M. Belgrand's opinion, with the Glacial period, and he considers that it was brought suddenly to a close with the low-level gravels. To this Quaternary period the peat deposits immediately succeed, owing, as the author ingeniously suggests, to the suddenly diminished rainfall leaving the rivers clearer and under conditions favourable for the growth of peat, which he shows never takes place in river valleys subject to frequent and heavy floods, but always in valleys where springs abound, and the floods are few and not turbulent.

The latter part of the work is occupied with a minute account of formation of gravel beds, and of the position of the Organic Remains, showing how all the features of those deposits are to be accounted for by ordinary river action, and that the mammalian remains are abundant precisely at those very places where a river with strong currents and numerous eddies would leave them. He endeavours to account also for the fact of all the bones of the larger animals being found in the coarser bottom beds of gravel, by the circumstance that these coarser beds were formed in those deeper water-channels along which only the larger carcases could have floated, and which were afterwards surmounted by those upper beds of sand and finer gravel, which he considers to be due to silting up (*alluvionnement*) of the channel where the river had changed its course to another channel. The brick earth or Loess is ascribed by him, as by English geologists, to river floods. But instead of considering it, as we do, to be produced by successive floods at all the various levels of the river, from the high to the low level, M. Belgrand admits but two levels, the high and the low, and that owing to a sudden elevation of the land, the excavation between these two levels was produced at once without intermediate stages. Consequently, he considers that the height of the Loess above these two levels marks in each case the rise of the flood waters. This, we think, is a weak point in his argument. According to his view, which he illustrates by a section, showing the range of the Loess up the hill slopes, he concludes that the floods of the low-level stage of the river rose, notwithstanding the width of the valley, to a height of 66ft., and during the high-level stage, to a height of 43ft., which give very much larger sectional areas for the river in flood than is otherwise necessary, and such as we conceive the area drained would have been insufficient to fill even with greatly larger rainfall. For, although the discharge of the Armançon may in a particular case of heavy rainfall have been so large as when multiplied by the whole area to give two-thirds of the required supply, still it is perfectly well known that the discharge by the main river never equals the sum of all its tributaries, and the discharge of the Seine at Paris on that occasion actually only rose to 1,250 cubic metres per second. There are besides beds of gravel on the slopes of Clichy towards Paris, and again on the slopes leading to Charenton distinct beds of gravel at intermediate levels, though of limited extent.

Thus, M. Belgrand ascribes the gravel beds and the Loess of the Seine Basin to old river action, referring the red loam alone of the higher plains above to diluvial causes, in opposition to the view usually received in France, according to which all these Drift beds are divided into the three diluvial deposits of *Diluvium gris*, *Diluvium rouge*, and *Limon* or *Loess*. As we have already expressed very similar views respecting the commonly accepted classification, we cordially agree with the author on this point.

The illustrations forming the second volume constitute a very interesting exhibition of the art of Photo-lithography. The execution varies a good deal, and there are plates which, though valuable for their truthfulness, are rather indistinct. Some of the representations of the Flint Implements are excellent. The work is somewhat large and costly; but as a copious record of facts, an ingenious statement of theory, and a reliable representation of specimens, this work of M. Belgrand will be greatly valued by all those who feel an interest in the remarkable phenomena connected with the present configuration of the country, the distribution of life during the Quaternary period, and especially with the evidence bearing on the Antiquity of Man.

J. P.

OUR BOOK SHELF

The Discovery of a New World of Being. By George Thomson. (Longmans, Green and Co., 1871.)

THE world discovered by this psychological Columbus is the "world of spirits," although he "disclaims all connection with so-called Spiritualists—a sect of modern times," whom he somewhat ungenerously "believes to be either dupes or knaves." Mr. Thomson believes that man consists of two "personalities," an animal personality or body, and a personality he calls spirit, which is the "knowing and conscious we," and which he believes to be as distinct from and as capable of being at almost any moment abstracted from the former as steam is from a steam-engine. Indeed, this latter phenomenon takes place every time the body "goes to sleep," to use the vulgar phrase; for Mr. Thomson believes that the "animal life never sleeps, and cannot sleep, and that to say or think that it, or any other life, can sleep, in the popular sense of the word, is the most glaring absurdity that ever has had possession of the human mind." "What is meant properly by sleep," he goes on to say, "is simply the abstraction or withdrawal of the influence of a being, a spirit, from a being, an animal, the leaving of a servant to itself, from the influence of its lord and master." Mr. Thomson explains the phenomenon of dreaming to be the struggles of this "being, a spirit," to get out of and back into the house of its servant, the body. The frequently

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unpleasant consequences of a late supper might have led Mr. Thomson one step further, and suggested to him the probable habitat of the spirit when embodied. How brimful of meaning to Mr. Thomson, then, must be Shakespeare's well-known utterance—"We are such stuff as dreams are made of." The particular merit which he claims for himself as a discoverer is, that he has realised to himself this spirit-world "predicted of old to be in existence," become conscious of himself as a "spirit in the world of spirits," clearly distinct, "in rounded belief," as he puts it, from that other entity, the body; and he declares that any one may make this awful discovery for himself if he only has "faith," shuts himself off from the outer world, and ponders long enough and with sufficient intensity. If our author is really in earnest—and we cannot but think he is—in trying to fathom the mystery of life and of consciousness, we recommend him to approach the subject unprejudicedly from the side of physiology; for so long as a psychologist concerns himself with the phenomena of his "inner consciousness" alone, and neglects the facts of his "outer man," his work is less than half done, and he is as likely to succeed in arriving at the whole truth as Columbus would have been in discovering America, had he contented himself with studying charts and staring longingly across the Atlantic for forty years.

On the Elevation of Mountains by Lateral Pressure; its Cause, and the Amount of it, with a Speculation on the Origin of Volcanic Action. By Rev. O. Fisher, M.A., F.G.S, &c. (From the Trans. of Camb. Phil. Soc. Vol. xi. part iii.)

THIS paper is of considerable interest as bearing upon the question of the internal condition of the earth. Mr. Fisher is of opinion that the elevation of mountain chains and the phenomena of volcanoes can both be accounted for on the hypothesis that the earth is solid. He conceives that "if a sufficient loss of heat has happened since the stratified rocks were formed, to cause a slight diminution in the volume of the earth, then the outer layer will have become too large, and will have had to accommodate itself to the reduced spheroid; and the lateral pressure caused by the resulting failure of support will have given rise to those foldings which have produced mountain ranges;" and an attempt is made by the author to "estimate the lateral pressure which would arise in the outer strata of the earth under such circumstances." Referring to the results obtained by Archdeacon Pratt in India, which seem to show that the density of the earth's crust beneath mountain chains is less than in other places, the author thinks this is only what might have been expected upon the supposition that the elevation of these mountains is due to lateral pressure; for it is evident that the strata would to some extent be supported by the lateral pressure which upheaved them. Here then, he thinks, may be the origin of volcanoes:—Diminished vertical pressure will enable the interior layers of the crust to pass into a state of fusion, and, "if from an independent cause a partial passage towards the surface is opened for molten rock containing highly heated water, the fluid will convey to a level where the resistance is less the pressure existing at a lower depth, and the force necessary to complete a passage to the surface may be furnished by the pressure of the molten rock and by the steam contained within it." But, although Mr. Fisher believes that the elevation of mountain chains and the phenomena of volcanoes are both of them the result of the same fundamental causes, yet, he thinks, it would certainly be a mistake to regard elevation as the consequence of volcanic action. He does not see how subterraneous lakes of molten matter can account for the elongated form which trains of volcanoes like those of the Andes affect; nor how such lakes should have shifted about from one region to another at different geological epochs. His theory, however, offers an explanation of the elongated form assumed by chains of volcanoes—the shifting of volcanic activity to different regions at successive periods—the spasmodic character of volcanic action, and other volcanic phenomena.

J. G.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Placental Classification of Mammals

A REMARK made by Prof. Allen Thomson on this subject in a late number of NATURE induces me again to draw attention to some objections I offered to the placental classification in a review of Prof. Rolleston's "Forms of Animal Life" (NATURE, vol. i., p. 81). If this system fails to satisfy so sound a critic and so accomplished an anatomist as Dr. Thomson, there must be some serious deficiencies in it. No doubt De Blainville did good service in calling attention to the wide distinction of Marsupials and of Monotremes from other mammals; but his names, *Ornithodelphia* and *Didelphia*, are inappropriate, and even misleading, and the skeletal characters of these two groups furnish quite as important, and far more available, means of diagnosis.

It admits of question whether the divisions of the higher mammals, according to the same system, are the most natural, even if the placenta were the best organ by which to define them. It is true, as Prof. Huxley observes, that the singularities which ally the elephant with the Rodentia have been a matter of common remark since the days of Cuvier, but the placental classification requires us to find still more singular ties between the elephant and the Carnivora. On the other hand the Carnivora lead down by the seals to the true Cetacea, a line of connection broken by the placental arrangement; which is equally opposed to the more doubtful analogy of the whales with the Ruminants. And the third order with deciduous zonary placentation, the isolated genus *Hyrax*, whatever may be thought of its relations to Rodentia on the one hand and to Ungulata on the other, has at least more likeness to either than to elephants and cats. Again, the different placentation of Edentata may be held only an additional proof of the looseness of an order held together chiefly by negative characters, but if we break it up, shall we obtain a more natural or convenient arrangement by placing the sloths with the Ruminants, *Manis* with Cetacea and Perissodactyla, and *Orycteropus* with Primates?

No doubt embryological characters are justly regarded as the most important for revealing true affinities between animals. But the tenacity of hereditary transmission, which gives them this value, does not appear to belong to placental structure. The placenta is more a maternal than a fœtal organ, especially as to its deciduate or non-deciduate character, and should rather rank with organs like the mamma than with the yelk-sac and the amnion.

There are, moreover, many practical objections to the placental classification. The opportunities of obtaining knowledge on the subject are few, the investigation is not always easy, and it cannot be readily verified by subsequent observers.

But the most important objection to De Blainville's system is, that the perishable nature of the structures on which it is based renders it impossible to apply the criterion to fossil animals. It will probably be long before we shall have any notion of what a Sirenian placenta is like; it is only lately that we have learnt what is the real placentation of so common a creature as the rat, but we shall certainly never have the remotest idea of that of a megatherium, a zeuglodon, or a rhytina. So that if it be admitted—and surely no one will deny—that any classification of animals which is to be more than a mere aid to the memory, must include all known forms, recent or fossil, it follows that neither placenta, nor brain, nor any other soft part, can be of more than subordinate value in classification. On the other hand, it may be fairly maintained that there is no group of mammals, and scarcely one of the other Vertebrata, of undisputed importance, which cannot be completely defined by the characters of the skeleton.

It is, I venture to think, rather the authority of such illustrious names as Gegenbaur and Huxley than its own merits which have recommended the placental classification of mammals. If we regard the object of classification to be the setting forth of true genetic relationships, all characters must be included, and among

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